Continued Development and Testing of Dual-Frame Surveys of Fishing Effort

Testing a Dual-Frame, Mixed-Mode Survey Design

Final Report

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1. Executive Summary

The data collection design tested in this study was developed as a potential alternative to the Coastal Household Telephone Survey (CHTS), the current methodology used by NOAA Fisheries to estimate marine recreational fishing effort. The design is based upon the results of previous MRIP pilot studies, which demonstrated that mail surveys that sample from residential address frames and state angler license databases provide greater coverage and result in higher response rates than the CHTS. The objectives of the study were to; 1) evaluate the feasibility of the data collection design as a potential alternative to the CHTS, 2) characterize the effects of data collection mode, including telephone and mail, on response rates, timeliness and survey measures, and 3) assess the survey coverage of state angler license databases.

Results from the study continue to demonstrate that mail survey designs are feasible for collecting recreational fishing data and estimating fishing effort. Final response rates for the mail survey component of the study were higher than the telephone component and eclipse telephone survey response rates after about three weeks of data collection. In addition, preliminary estimates derived from early mail survey returns were not significantly different from final estimates, demonstrating that a mail survey can generate valid preliminary estimates within the current estimation schedule for the CHTS.

The impact of data collection mode on survey measures requires further investigation. We hypothesize that differences between telephone and mail estimates are the result of differential recall and coverage errors, and suggest that telephone samples are more susceptible to bias resulting from these errors. This hypothesis is speculative and was not tested in the present study.

As in previous studies, total fishing effort estimates - number of angler trips - generated solely from license samples were considerably lower than estimates generated from samples of residential addresses (ABS or address-based samples). We explored differences between license and ABS effort estimates in terms of survey error and suggest that under-coverage of license frames resulting from license exemptions and unlicensed fishing is the most likely source of the differences. Subsequently, we conclude that within the South Atlantic region, sampling exclusively from state license databases is likely to result in an underestimate of total fishing effort.

Finally, matching errors, resulting in misclassification of sampling domains (e.g., license only; ABS only, both license and ABS), continue to be a challenge and are likely to result in biased estimates. These errors must be addressed, either through improved matching procedures or development of alternative estimators that reduce the impact of misclassification errors, before dual-frame designs can be considered as an alternative to the CHTS.

The following are specific recommendations and conclusions:

1. Mail surveys are a feasible alternative to telephone surveys for collecting recreational fishing effort data. Mail surveys result in higher response rates than telephone surveys, and preliminary mail survey estimates can be generated in a timeframe consistent with the current CHTS estimation schedule.
2. Incorporating angler license databases into data collection designs increases the efficiency of recreational fishing surveys. While sample frames derived from license databases may be incomplete due to unlicensed fishing activity, samples of licensed individuals and households with licensed anglers are much more likely to report fishing than general population samples. Supplementing household samples with information from license databases should increase the efficiency of data collection while maintaining coverage of the entire population.
3. Frame matching errors are a recurring problem and potential source of bias in dual-frame sampling designs. Frame standards, which were implemented during the study, will help minimize matching errors.
4. Further study is needed to better understand the impact of data collection mode on survey measures. However, we hypothesize that differences between telephone and mail estimates are the result of recall error and coverage error, and that telephone samples are more susceptible to biases resulting from these errors. These hypotheses were not tested in the present study.
5. In the South Atlantic region, it is currently not feasible to sample exclusively from state license databases. In the present study, total effort estimates derived from license samples were considerably lower than ABS estimates. We attribute these differences to coverage error resulting from license exemptions and unlicensed fishing activity.
6. Cash incentives provide a substantial boost in response rates for mail surveys and should be considered in any mail survey design.
7. The single-phase ABS design with screening prior to data collection proposed by Brick et al. (2012) should be tested as an alternative to dual-frame designs. Such a design is not susceptible to bias resulting from matching error and is likely to result in higher response rates than the two-phase design.

2. Introduction

In a dual-frame survey, independent samples are selected from two sample frames, and the resultant data are combined to estimate population totals or means. Often, the goal of a dual-frame design is to maximize both efficiency and coverage, particularly when sampling a rare population (Lohr 2009). Previous MRIP pilot studies (Andrews et al. 2010; Brick et al. 2012a, Brick et al. 2012b) have demonstrated the benefits of dual-frame, mail survey designs for sampling recreational anglers and collecting recreational fishing effort data. The dual-frame sampling design, which samples from comprehensive lists of residential addresses and state databases of licensed anglers, provides greater coverage and efficiency than the random-digit-dial (RDD) frame used for the Coastal Household Telephone Survey (CHTS), and mail surveys result in considerably higher response rates and may be less susceptible to measurement errors than telephone interviews (Brick et al. 2012b).

Despite these benefits, concerns persist that a mail survey cannot satisfy customer needs for timely estimates. The dual-frame, mixed-mode survey was designed to address these concerns by measuring the impact of data collection mode on response rates, survey measures, and the timeliness of data collection, in a controlled, experimental setting. In many mixed-mode designs, sample units are offered a choice of reporting mode, either concurrently or sequentially, with a goal of reducing coverage bias, nonresponse and/or cost (de Leeuw 2005). In the current study, sample units were not offered a choice of reporting mode, but were allocated into exclusive telephone or mail treatments, which allowed direct comparisons between modes on measures of survey quality. The goal of this design was to assess differences between telephone and mail modes in terms of response rates, timeliness and survey measures.

The objectives of the dual-frame mixed-mode pilot study were to, 1) continue to test and document the general feasibility, including both benefits and limitations, of dual-frame, mail survey designs for collecting recreational fishing effort data, 2) examine the impact of data collection mode (mail and telephone) on survey response and measurement, 3) determine the timeframe in which a mail survey can deliver reliable estimates, 4) evaluate the completeness and quality of state angler license databases in the study states, and 5) test for nonresponse bias in mail survey designs.

3. Methods

3.1. Sampling Design

The dual-frame mixed-mode survey (henceforth referred to as the "fishing effort survey") was conducted in four states (NC, SC, GA and FL) in the South Atlantic Region. In each state, fishing effort data, including the number of trips by fishing mode, were collected for six independent two-month reference waves, beginning with wave 1 (Jan/Feb), 2012, and continuing through wave 6 (Nov/Dec), 2012. The survey utilized a dual-frame design that sampled from state databases of licensed saltwater anglers (license frame) and residential address frames (address-based samples or ABS). The union of the license and ABS frames consists of three domains; households in the address frame but not in the license frame, households in the license frame but not the address frame, and households in both frames (overlap domain).

The ABS frame is derived from the United States Postal Service, Delivery Sequence File (DSF), and includes all residential addresses within the study area[1]. For each state and wave, sampling was stratified at the county level into coastal and non-coastal strata[2]. Geographic stratification within states provides an opportunity to sample strata at different rates and subsequently increase the efficiency of data collection. For example, historical estimates from the Marine Recreational Fisheries Statistics Survey (MRFSS) demonstrate that 65-90% of recreational saltwater fishing trips in the study states are taken by residents of coastal counties within those states.

Sampling from the ABS frame was conducted through a two-phase data collection model. In the first phase, a household screener questionnaire is mailed to a sample of residential addresses. The questionnaire (Appendix A) identifies eligible anglers – adult residents who fished during the previous year or are likely to fish during the next three months. The screener questionnaire collects information for up three adults per sampled address. For each wave and state, a random sample of 6,000 addresses is selected from the ABS frame and matched, by address and telephone number, to the state's directory of licensed anglers. This matching identifies the domain for each sampled address – addresses on both frames (matched sample) and addresses on only the address frame (unmatched sample). Table 1 provides the first-phase ABS sample sizes by state and sub-state stratum for each reference wave.

[1] ABS samples were purchased from a commercial vendor licensed by the USPS to distribute the computerized delivery sequence file. The sample included "residential only" and "primary residential with some business" addresses.
[2] Counties included in the coastal and noncoastal strata varied by wave. During waves 1, 2 and 6, all counties within 25 miles of the coast are included in the coastal stratum. In waves 3-5, counties within 50 miles of the coast are included in the coastal stratum.

Table 1. Number of sampled addresses by stratum and survey wave for the first-phase ABS sampling.

Stratum	Wave 1	Wave 2	Wave 3	Wave 4	Wave 5	Wave 6
North Carolina	6,000	6,000	6,000	6,000	6,000	6,000
Coastal	1,199	1,873	2,634	2,619	2,618	1,873
Non-Coastal	4,801	4,127	3,366	3,381	3,382	4,127
South Carolina	6,000	6,000	6,000	6,000	6,000	6,000
Coastal	1,963	2,856	2,856	2,856	2,856	2,856
Non-Coastal	4,037	3,144	3,144	3,144	3,144	3,144
Georgia	6,000	6,000	6,000	6,000	6,000	6,000
Coastal	388	791	791	791	791	791
Non-Coastal	5,612	5,209	5,209	5,209	5,209	5,209
Florida	6,000	6,000	6,000	6,000	6,000	6,000
Total	24,000	24,000	24,000	24,000	24,000	24,000

All eligible adults identified in the screener phase were sampled for the second-phase or topical survey, which collects details about recreational saltwater fishing activity that occurred during two-month reference waves. To permit comparisons between telephone and mail data collection modes, the topical ABS sample, which consists of individual anglers rather than households, was randomly distributed between telephone and mail treatments after allocating sample with no known telephone number to the mail treatment[3]. Table 2 provides the topical ABS sample sizes by state and stratum for each reference wave.

Table 2. Second-phase ABS sample sizes by stratum and survey wave[4].

Stratum	Wave 1	Wave 2	Wave 3	Wave 4	Wave 5	Wave 6
North Carolina	416	465	441	585	600	619
Coastal	147	221	249	350	331	266
Non-Coastal	269	244	192	235	269	353
South Carolina	414	508	456	614	546	599
Coastal	221	340	299	396	345	409
Non-Coastal	193	168	157	218	201	190
Georgia	242	282	298	454	348	415
Coastal	34	77	76	113	91	105
Non-Coastal	208	205	222	341	257	310
Florida	549	533	560	743	884	920
Total	1,621	1,788	1,755	2,396	2,378	2,553

[3] Allocation to telephone and mail treatments is done at the household level so that multiple individuals at the same address do not receive survey requests from different modes.

[4] Sample sizes reflect 1st phase ABS sample sizes, 1st phase ABS household response rates, and the eligibility rates of individuals within responding households.

The license frames are derived from state databases of adults who were licensed to participate in saltwater fishing in the study states between the beginning of each reference wave and the time the sample frame is created, approximately one month prior to the end of each wave. Sampling, which is conducted in a single phase, is stratified by state (state of licensure) and sub-state region of residence. License frame sampling also includes a stratum for licensed anglers who reside outside the state of licensure (nonresident anglers). As with the topical ABS sample, the license sample is randomly distributed between telephone and mail treatments, and sampled individuals are asked to describe saltwater fishing activity that occurred during the reference wave. Table 3 provides license frame sample sizes by state and stratum for each reference wave.

Table 3. License frame sample sizes by stratum and survey wave.

Stratum	Wave 1	Wave 2	Wave 3	Wave 4	Wave 5	Wave 6
North Carolina	1,072	1,072	1,072	1,072	1,070	1,072
Coastal	448	446	618	611	687	478
Non-Coastal	565	577	405	395	349	531
Nonresident	59	49	49	66	34	63
South Carolina	1,072	1,071	1,072	1,072	1,072	1,072
Coastal	458	450	540	440	440	435
Non-Coastal	581	567	491	613	631	634
Nonresident	33	54	41	19	1	3
Georgia	1,072	1,072	1,072	1,072	1,072	1,072
Coastal	162	171	223	268	146	138
Non-Coastal	890	882	824	780	920	915
Nonresident	20	19	25	24	6	19
Florida	1,071	1,072	1,072	1,072	1,065	1,072
Coastal	931	927	937	1008	940	976
Nonresident	140	145	135	64	125	96
Total	4,287	4,287	4,288	4,288	4,279	4,288

3.2. Data Collection Procedures

The purpose of the ABS screener survey was to identify likely saltwater anglers from among the general household population. To accomplish this, the screener instrument asked about past and likely future participation in a variety of outdoor recreation activities, including saltwater fishing. All residents who reported saltwater fishing during the previous 12 months or likely participation in saltwater fishing during the next three months were eligible for the topical sample. Initially, the screener instrument also requested a telephone number that could be used for the follow-up, topical survey. We suspected that asking for a telephone number had a negative impact on screener response rates, so we eliminated this question following wave 4[5].

Screening of the ABS sample begins at the start of each reference wave to allow for adequate time to compile the topical sample (i.e., identify eligible anglers). Sampled addresses receive multiple mailings, including an initial mailing of the screener questionnaire, a reminder postcard one week after the initial mailing, and a second mailing of the screener questionnaire to nonrespondents two weeks after the mailing of the postcard. Screener questionnaire mailings, which are administered via regular, first class mail, include a cover letter stating the purpose and importance of the survey, the survey instrument and a post-paid return envelope. A $1.00 cash incentive was included in the initial survey mailing beginning with wave 4 and continuing through wave 6. When available, sampled addresses were augmented with the name of a household resident, and survey packets were mailed to the named individual[6]. This was a substantive design change from previous MRIP effort survey pilot studies, in which first phase ABS mailings were addressed to "state resident", for example, NC resident (Andrews et al. 2010; Brick et al. 2012b).

The topical survey is administered through either Computer Assisted Telephone Interviewing (CATI) or completed and returned mail questionnaires. Telephone numbers were obtained either from the screener survey, as described above, or through commercial directory matching[7]. Regardless of data collection mode, data collection is retrospective for the most recent two-month reference wave, and survey questions for each mode were developed to be as similar as possible to minimize the potential for differential interpretation of the meaning of questions.

The first mailing for the mail treatment begins one week prior to the end of each reference wave. This ensures that respondents receive survey materials by the end of the wave. The sequence of mailings includes an initial mailing of the topical questionnaire, a reminder postcard one week after the initial mailing, and a second mailing of the topical questionnaire to nonrespondents two weeks after the mailing of the postcard. The initial mailing is administered via USPS Priority Mail. All subsequent mailings are administered via regular, first class mail. In previous studies (Andrews et al. 2010; Brick et al. 2012), a $1.00 cash incentive was included in the initial mailing for both the license samples and the second phase ABS samples. However, we chose not to include incentives for these samples in the present study.

All telephone interviewing for the topical survey begins on the day following the end of each wave and continues for 10-11 days. Calls are scheduled among days and times such that the

[5] Dropping the telephone question had a very modest impact on screener response weights that was far outweighed by the negative impact on the quality of telephone numbers in the topical phase.
[6] The ABS vendor attempted to identify a name for each sampled address.
[7] For waves 5 and 6, telephone number was obtained only through commercial directory matching.

likelihood of completing an interview is maximized. The telephone instrument is designed to collect information about saltwater fishing activity that occurred during the wave.

3.3. Data Delivery

To determine if a mail survey design can produce unbiased and reasonably precise estimates within the current timeframe of the Coastal Household Telephone (CHTS), we produced estimates generated from both partial and complete survey data. Partial data included complete data for the CATI treatment and mail surveys that were returned within 15-21 days[8] from the initial survey mailing.

4. Key Findings

4.1. Survey Eligibility and Efficiency

Over the past three decades, the CHTS has demonstrated that during a given two-month period, residents in fewer than 10% of households participate in recreational saltwater fishing. Subsequently, recreational saltwater anglers are generally considered a rare population and are inefficient to sample using traditional household survey designs (Lohr 2009). Multiple-frame designs that sample from both partial lists of likely participants and general population frames are often used to increase the efficiency of data collection while maintaining coverage of the entire population (Kalton and Anderson 1986; Lohr 2009).

In the present study, we attempted to increase efficiency by sampling from lists of licensed saltwater anglers. We also identified households from within the general population that included at least one licensed saltwater angler by matching ABS samples to saltwater license directories. While this matching was intended to support estimation – dual-frame estimation requires that the domain of sample units be known – it also provided an opportunity to compare fishing activity among domains. Table 4 compares overall fishing prevalence (percentage of respondents over all phases of sampling that reported fishing during the reference wave) overall, by state and by wave, for license samples and the matched and unmatched domains of the ABS samples. Overall, fishing prevalence was considerably higher in the license sample (37.2%) than in either the matched (21.9%) or unmatched (6.6%) ABS samples. The relatively high rate of reported fishing activity in the license sample demonstrates that sampling directly from license databases can be much more efficient than screening general household samples for anglers. Similarly, sampling from the matched domain is more efficient than sampling from the unmatched domain. We expected fishing activity in the matched ABS and license samples to be the same – theoretically, the two samples are from the same population. We attribute differences

[8] For wave 1, preliminary data included mail surveys that were returned within 15 days of the initial survey mailing. For subsequent waves, preliminary data included mail survey returned within 21 days of the initial mailing.

in estimates between the samples to matching errors, which are described more fully below. Matching errors, resulting in misclassification of sample into domains, can result in biased estimates and are a potential limitation of the dual-frame design (Lohr 2009). However, the results presented here demonstrate the potential of both license frames and frame matching for increasing the efficiency of angler surveys.

4.2. Frame Matching and Domain Identification

Previous MRIP pilot studies identified frame-matching errors as a potential source of bias in dual-frame sampling designs (Andrews et al. 2010; Brick et al. 2012b). Matching errors occur because the fields used to match ABS samples to license frames, including address and telephone number, may be missing, incomplete or inaccurate on one or more of the frames. The result of matching errors is that domains for some sample units are misclassified, and sample weights are not adjusted to reflect selection probabilities; some units are inappropriately excluded from the overlap domain and are not down-weighted (we refer to this as under-matching), and some units are inappropriately included in the overlap and are down-weighted when they should not be (over-matching).

Errors resulting from both under-matching and over-matching have been reported for dual-frame fishing surveys (Brick et al., 2012b; Andrews et al., 2010). It's interesting to note that both types of errors are likely to have the same net effect; overestimation of fishing effort. In the case of under-matching, sample units with licensed anglers, which are more likely to report fishing, are excluded from the overlap domain and are not appropriately down-weighted[9]. This effectively results in over-representation of fishing households in the sample. In contrast, over-matching results in under-representation of non-fishing households; sample units without licensed anglers, which are less likely to report fishing, are included in the overlap domain and are inappropriately down-weighted.

In the present study, 7.7% of sampled addresses matched to the state license databases. In other words, an estimated 7.7% of households included at least one resident who was licensed to fish within his or her state of residence during the reference waves. Matching rates were highest in South Carolina (9.9%), followed by Florida (8.6%), North Carolina (7.9%) and Georgia (4.5%).

We identified matching errors by comparing the estimated number of households with licensed anglers, derived from ABS samples, to actual counts of unique addresses on the license frames (Table 5). The accuracy of matching varied by state, but in nearly every comparison, estimated values were significantly higher than actual counts, suggesting that over-matching was the dominant error. The mechanism for over-matching appears to be related to the criteria used to identify matching cases. Specifically, ABS sample cases were identified as matches if at least one of the following conditions was satisfied: 1) the ABS record contained the same primary

[9] Households in the overlap domain are down-weighted because they can be selected from each of the two sample frames; i.e. they have higher selection probabilities.

street address, zip code, and state of residence as a record in the corresponding license frame; or 2) the ABS record contained the same telephone number (obtained through commercial directory matching), zip code, and state of residence as a record in the corresponding license frame. Secondary street address, which is more specific than primary street address and includes apartment numbers, was excluded from the matching criteria. Subsequently, any ABS sample unit that was part of a multi-unit dwelling, such as an apartment building or condominium, was identified as a match if the license frame included anyone who resided at the multi-unit dwelling. A cursory review of records on the license frames that matched to an ABS sample unit revealed an abundance of apartments, suggesting that the exclusion of secondary address from the matching criteria was a significant source of over-matching. We excluded secondary address from the matching criteria because information within the field was inconsistently reported and formatted, and including the field would likely have resulted in significant under-matching. Variation in the extent of matching errors among states may be due to the relative occurrence of multi-unit dwellings within the individual states. According to numbers reported by the Census Bureau (http://quickfacts.census.gov/qfd/index.html), the percentage of total housing units that are within multi-dwelling structures is higher in FL (29.9%) and GA (20.5%) than in NC (17.0%) and SC (17.6%). Coincidentally, the extent of over-matching errors is greater in FL and GA than in NC and SC.

Because telephone number is included in the matching criteria, exclusions or inaccuracies in this field can also contribute to matching errors. We observed that 56.7% of records on the GA license frame were missing a telephone number, followed by SC (27.2%), NC (15.3%) and FL (8%). We assessed the accuracy of telephone numbers by examining final dispositions of sample units within the CATI treatment and observed less variation among states; in FL, 18.7% of sample cases were classified as "bad number"[10], followed by NC (16.8%), SC (16.5%), and GA (14.0%). While these results suggest that missing and incorrect telephone numbers are a likely source of error in the matching process, it's unlikely that these errors contributed to the observed over-matching. Rather, we would expect errors in telephone number, which is the most specific level of resolution within the telephone matching condition, to result in under-matching. It's difficult to quantify the impact of telephone number errors on match rates in the current study due to the extent over-matching resulting from the address match.

It's noteworthy that the magnitude of over-matching increased between waves 3 and 4. While the matching protocols did not change during this period, procedures used to validate and standardize address records on the license sample frames were implemented between waves 3 and 4. Further examination is needed to understand how these procedures impacted the matching process.

[10] Bad numbers included dedicated fax lines, non-working numbers, non-working/disconnected numbers, temporarily out-of-service, and business numbers.

The results from this study identify several challenges that should be addressed if dual-frame sampling designs are to be considered as a possible alternative to the CHTS. First, estimators for dual-frame designs assume that domain membership of sample units is known and accurate. If this assumption is false, then population estimates may be biased (Lohr 2009). The results of this and prior studies (Andrews et al. 2010; Brick et al. 2012b) demonstrate that defining domain membership, whether through a priori matching of sample frames or responses to survey questions, is complicated and subject to error. These errors must be addressed, either through improved matching procedures or development of alternative estimators that reduce the impact of misclassification errors. Next, the decision to exclude secondary addresses from the matching criteria highlights the need to improve the quality of address information on the license frames. Implementing USPS postal addressing standards (USPS 2010) will increase matching accuracy and provide maximum flexibility for selecting matching criteria. Finally, the quality of telephone numbers must improve if license databases are to be used for sampling purposes. Missing and non-working telephone numbers result in matching errors in dual-frame designs, whether data are collected through mail surveys or telephone interviews. In addition, nonresponse and/or non-coverage resulting from missing or inaccurate telephone numbers introduce the potential for bias in telephone surveys, regardless of the sample design.

4.3. Response Rates and Timeliness

An objective of this study was to assess the feasibility of the mail survey design in terms of response rates and the timeliness of generating estimates. The current pilot study achieved screener response rates of 37.3% and topical survey response rates of 46.4% for an overall response rate of 17.3% for the ABS sample (Table 6). The overall response rate for the license sample was 33.1%. While lower than anticipated, these rates exceed the CHTS response rate of 13.3% for the same geographic region and time period. These response rates are considerably lower than the rates achieved in previous studies that tested similar mail survey designs. Andrews et al. (2010) reported response rates of 58.2% for license samples and overall response rates of 33.1% for ABS samples, including screener response rates of 45.6% and topical survey response rates of 72.5%. Brick et al. (2012b) reported similar rates; 49.3% for license samples and 30.5% for ABS samples, including screener response rates of 46.7% and topical survey response rates of 65.4%.

We suggest that notable design features and design modifications contributed to the lower-than-expected response rates. First, both Andrews et al. (2010) and Brick et al. (2012a) included a third mailing of the topical instrument, which increased topical survey response rates by as much as 10 percentage points[11]. Future administration of mail survey designs should consider the tradeoff between the costs of additional mailings, which can be considerable, and the benefits of higher response rates. Second, previous tests of the dual-frame mail survey design addressed all

[11] Brick et al. (2012b) included an experimental treatment to quantify the impact of a third mailing on response rates.

ABS screener mailings to “State Resident”. In the present study, we attempted to augment ABS samples, through commercial directory matching, with the name of a household resident and addressed survey materials to that individual when such information was available. This design feature was intended to increase response rates. However, Link et al. (2008) demonstrated that including a surname on survey materials resulted in lower response rates and suggested that household members may be more likely to discard survey materials if name matching is inaccurate. A third design feature that likely had a considerable impact on response rates was the use of incentives. Both Andrews et al. (2010) and Brick et al. (2012b) included a $1.00 cash incentives in all initial survey mailings – ABS screener survey, ABS and license topical surveys. The present study did not include incentives for any survey mailing until wave 4, when a $1.00 cash incentive was added to the initial mailing of the ABS screener survey. Subsequently, response rates for the screener survey increased from an average of 31.3% for waves 1-3 to an average of 43.7% for waves 4-6, rates very similar to those reported previously (Figure 1). The incentive also decreased the response time for the screener survey (Figure 2). The median response times with and without the incentive were 14 days and 20 days, respectively. We suspect that differences in overall response rates between the current and previous studies would have been further minimized had we included an incentive in the topical survey mailings. Finally, the use of CATI interviewing in the present study may have impacted overall response rates in two ways. First, the response rates reported for the present study include both mail and CATI data collection for the topical ABS and license surveys. Excluding the CATI treatment from the response rate increases the overall rates to 22.0% and 38.1% for the ABS and license samples, respectively. Second, including a CATI treatment likely impacted the characteristics of the mail sample, which may have resulted in lower response rates. As mentioned, topical ABS sample was randomly assigned to mail or CATI treatments after sample with no known telephone number was allocated to the mail treatment. As a result, the mail sample included a relatively lower proportion of sample units with a known telephone number than would be expected in a randomly selected mail sample. Previous studies (Hagedorn et al. 2009; Brick et al. 2011) have demonstrated that response rates are higher for samples that can be matched to a telephone number. We suggest that sampling constraints imposed by the dual-mode design had a negative impact on response rates. We explored this hypothesis by comparing response rates for the full topical ABS mail sample to rates for the portion of sample that include a matched telephone number and found that overall response rates for the telephone matched sample were 2.8 percentage points higher than rates for the full sample (22% vs. 19.2%). We further suggest that the decision to remove the telephone question from the screener instrument following wave 4 exacerbated this impact. The intent of this design modification was to increase response rates at the screener phase of data collection, and to a modest extent, this appears to have been successful as screener response rates increased slightly between wave 4 (43.2%) and waves 5 (45.1%) and 6 (44.1%). However, a more pronounced effect was observed at the topical phase, where response rates, including both modes of data collection, decreased from an average of 49% for waves 1-4 to 41% for waves 5-6. An obvious explanation for this decrease is that self-reported telephone numbers provided by screener respondents are more accurate than numbers obtained through directory matching. This explanation is supported by the response rates achieved in the topical ABS CATI treatment, which decreased from an average of 41% during waves 1-4 to 33% for waves 5-6. A review of final CATI dispositions reveals that telephone numbers obtained through directory matching are more likely than self-reported numbers to be classified as "bad numbers" (18.1% vs. 9.7%). Response rates in the topical ABS mail treatment also decreased after the telephone question was eliminated from the screener instrument. Because the telephone question was eliminated, a larger percentage of the total topical ABS sample, sample units with no known telephone number, was automatically assigned to the mail treatment. Consequently, the wave 5 and wave 6 mail samples included relatively more units without known telephone numbers than the prior waves, which resulted in even lower response rates, as described above. Several of these explanations are anecdotal and would require further investigation to quantify the impact of specific design features on response rates. However, it seems likely that modifications to the data collection design contributed to the differences in response rates between the current and previous studies.

The present study included comparisons between mail and CATI to quantify differences in response rates between modes and assess the timeliness of a mail survey design. Consistent with observations from recent studies (Link et al. 2008) final response rates for the mail treatment were consistently higher than the CATI treatment in both the ABS and license samples (Table 7). Final mail response rates eclipsed CATI response rates by 4.8% and 10.7% for the ABS and license samples, respectively[12]. Differences in response rates between CATI and mail were even larger when the comparison is limited to ABS sample cases for which a telephone number could be located through directory matching. CATI and mail response rates for these cases, which represent the population that can be covered by a telephone survey, were 13.4% and 22.0%, respectively. While response rates alone cannot predict or measure nonresponse bias, a higher response rate decreases the risk of nonresponse bias (Groves 2006). Furthermore, mail survey response rates for both the topical ABS and license samples eclipsed CATI response rates after about three weeks of data collection (Figure 3). This demonstrates that a mail survey can collect a similar amount of data as a telephone survey in a relatively short timeframe, and suggests that preliminary estimates, generated from partial mail survey data, can be produced in a timeframe consistent with the current CHTS data collection and estimation schedule[13].

A concern about using partial data to generate estimates is that mail survey respondents who complete the survey within a few weeks of receiving the questionnaire may be different from those who wait longer to return the survey. This type of nonresponse bias has been documented previously for populations of hunters (Filion 1976) and anglers (Fisher 1996). In both cases,

[12] For the ABS samples, we compared overall CATI and mail response rates, which is the product of screener response rates and topical survey response rates. The screener survey was administered via mail for both CATI and mail topical treatments.
[13] CHTS effort estimates are generally available 45 days following the completion of the reference wave.

those who responded later, after additional attempts to complete surveys, were less likely to have participated in the measured behavior.

To assess differences in fishing activity between early and late responders, we compared estimates of survey measures, including fishing prevalence, and mean private boat and shore trips per angler, between early responders (those who responded within 3 weeks of the first survey mailing) and all responders (Table 8). There were no significant differences between preliminary and final estimates overall or within states for any of the survey measures, demonstrating that "early" and "late" responders are not substantially different in terms of reported fishing activity. While this result does not suggest that the overall mail survey design is immune to nonresponse bias, it does demonstrate that point estimates derived from preliminary mail survey data are not likely to be substantially different from final estimates, produced after data collection has been completed. This result is consistent with the observation that mail surveys are feasible if "data are needed in a couple of weeks" (de Leeuw 2008), and provides further evidence that a mail survey design is a feasible alternative to telephone surveys for producing recreational fishing statistics in a timely manner. A caveat to this conclusion is that final mail survey estimates are likely to be more precise than preliminary estimates as the number of completed surveys (i.e., sample size) increases.

4.4. Mode Effects on Survey Measures

When considering multiple data collection modes, or switching modes in an ongoing survey, care must be taken to ensure that survey measures are not impacted by the reporting mechanism. Dillman et al. (2009) and de Leeuw (2005) suggest that different data collection modes can result in very different responses, particularly when comparing visual vs. aural or interviewer-administered vs. self-administered modes. We attempted to minimize the impact of survey mode on responses by making the instruments as similar as possible, keeping the survey relatively short and straightforward and avoiding categorical responses that could result in primacy or recency effects (Dillman et al. 2009). Despite these efforts, we observed differences between CATI and mail treatments for some survey measures (Table 9). Specifically, in the license sample, estimates of fishing prevalence in the mail treatment were significantly higher than CATI estimates overall and in SC and FL. This result is consistent with the findings of Brick et al. (2012b), who reported that mail surveys resulted in larger estimates of active anglers[14] than telephone surveys for independent samples selected from a frame of licensed saltwater anglers. Brick et al. (2012b) hypothesized that measurement errors resulting from differences in screening approaches were responsible for the observed differences between telephone and mail interviews and introduced the concept of a "gatekeeper effect", where the initial respondent to a telephone interview provides inaccurate responses to screening questions – in this case, questions about fishing, effectively screening the household or individual out of the eligible sample. This

[14] Estimates of active anglers are the product of fishing prevalence (% of respondents reporting fishing) and the size of the sample frame (N).

hypothesis is supported by results from a follow-up telephone survey of licensed anglers that measured more household-level fishing activity when screener questions[15] asking about fishing activity were administered to the sampled angler than when they were administered to the person who answered the phone (Andrews and Foster, unpublished). In the current mixed-mode study, surveys targeted the sampled angler; survey materials were addressed to a named individual in the mail treatment, and telephone interviewers asked for the sampled angler by name in the CATI treatment. In addition, proxy reporting was not permitted for either treatment, although this is difficult to control in a mail survey. We would expect these design features to minimize any bias resulting from a gatekeeper effect, as the respondent, who is also likely to be the sampled angler, is likely to know about his or her own fishing activity.

An alternative explanation for the observed differences relates to the tasks imposed upon respondents in the respective treatments. Sampling from the license frames is conducted in a single phase with no advance notice. In the mail treatment, the respondent is in control of the interview. Subsequently, respondents have time to carefully consider the questions and review schedules or calendars that may help respond to the survey request (de Leeuw 2005). In fact, the mail questionnaire includes a recall aid in the form of a calendar depicting the two-month reference wave next to the questions about fishing activity. In telephone interviews, the interviewer is in control, and respondents are generally expected to answer questions immediately, without the benefit of memory cues or aided recall. Cursory cognitive processing resulting from the nature of the telephone interview may result in recall error and fewer reports of fishing activity (de Leeuw 2005).

A final possibility is that differences between CATI and mail treatments are the result of differential nonresponse bias between the data collection modes. For example, individuals who didn't fish during a reference wave may be less inclined to respond to a mail survey than a telephone survey. Brick et al. (2012b) considered this type of nonresponse bias, referred to as "avidity bias", when exploring differences between telephone and mail estimates generated from samples of licensed anglers and concluded that avidity bias is not a significant concern for samples of licensed anglers. Mail survey response rates in the current study are considerably lower than those observed by Brick et al. (2012b) due to design changes described previously. However, as previously mentioned, we did not observe differences in reported fishing activity between early and late responders, which suggests that avid anglers are no more inclined to respond to the survey than less avid or non-anglers.

The differences for fishing prevalence between CATI and mail license samples contrast with results from the ABS samples, where differences between CATI and mail treatments were neither significant, nor systematic for fishing prevalence. The questionnaires used for the license

[15] Screening questions sequentially ask how many people in the household fish, how many people in the household fished during the previous 12 months, and how many people in the household fished during the previous 2 months.

and topical ABS samples were identical. However, the screening procedures for identifying anglers were considerably different for the two frames. For the ABS sample, screening for anglers was conducted in a completely separate phase via a household mail survey. Subsequently, individuals included in the topical survey sample, in both CATI and mail treatments, were previously exposed to a survey from the same sponsor and expressed previous participation or likely future participation in recreational saltwater fishing. Receipt or completion of the screener survey may have provided a memorable event or fixed point against which subsequent fishing behavior (i.e., fishing during the reference wave) was measured and reported in the topical survey. In fact, individuals in the topical ABS sample are reminded about their participation in the screener survey, either in the cover letter for the mail treatment or the introduction to the CATI interview. In this sense, the screener questionnaire may have served as a memory cue that aided in recall of fishing activity and muted or eliminated any effects of data collection mode on survey measures, not unlike the use of a bounded interview design (e.g., National Crime Victimization Survey; Consumer Expenditure Survey) (Neter and Waksberg 1964).

The results from the topical ABS survey support the suggestion that differential nonresponse bias does not contribute to the differences between data collection modes in the license samples for estimates of prevalence. The topical ABS and license samples are similar in that they were both selected from lists of likely saltwater fishing participants; licensed anglers for the license sample and self-reported anglers for the ABS sample[16]. Given their presumed interest in saltwater fishing, we might expect individuals on the two frames to have similar propensities to respond to the survey request, in which case, we would expect the impact of nonresponse bias on estimates to be consistent across sample frames. This also provides further evidence that the screening approach, which is the same across treatments in the topical ABS, but substantially different across treatments in the license survey, contributes to the differences in fishing prevalence in the license survey, as suggested by Brick et al. (2012b).

We also observed differences between CATI and mail for estimates of mean shore trips per angler, for those anglers that reported shore fishing during the wave. Previously, we suggested that CATI estimates of fishing prevalence may be susceptible to recall bias, and that the effect is likely to be greater in the license treatment, where the CATI interview is the first survey contact. The nature of the CATI interview may similarly limit respondents' ability to accurately recall the number of discrete fishing events that occurred during the reference wave, particularly if those events aren't especially memorable. We suggest that differences between modes in estimated fishing activity could be mainly limited to shore fishing because shore fishing is a less memorable than private boat fishing – private boat fishing generally requires a greater investment in both time and money, which is likely to increase the salience of the activity. However, we do not expect the effect of this recall bias to be limited to the ABS samples. In fact, based upon our earlier discussion, we expect recall bias to have a greater impact on the

[16] Some individuals in the ABS samples were also licensed to fish.

license samples, where respondents are asked to describe fishing activity, including enumeration or estimation of the number of fishing events, without the benefit of memory cues or a previous survey contact.

One possible explanation for this anomaly is that proxy reporting is permitted for the topical ABS CATI sample in some circumstances – proxy reporting is not permitted in the license sample[17]. We observed that, on average, proxy respondents reported fewer trips than self-respondents. However, proxy reporting accounted for less than 1.5% of total reporting, and eliminating proxy responses from our analysis did not significantly impact the outcomes. Based upon these results, we cannot conclude that the differential mode effects for the ABS and license samples are the result of proxy reporting, although the extent of proxy reporting may be larger than we are aware.

A second possibility is that differences in demographic characteristics, resulting from differential coverage of the two samples, are responsible for the differences in estimated shore fishing activity between CATI and mail treatments. Topical ABS samples are randomly distributed between CATI and mail treatments after sample units without a known telephone number are assigned to the mail treatment. This design could impact mode comparisons if sample units without telephone numbers, which are restricted to the mail treatment, fish more or less than sample units with telephone numbers. For example, Blumberg and Luke (2013) report that residents of wireless-only households are more likely to be younger and single than residents of households with landline telephones. Estimates of fishing effort could be impacted if characteristics such as these are correlated with recreational fishing activity. For waves 1-4, telephone number was obtained through the ABS screener survey – screener survey respondents were asked to provide a telephone number - as well as through commercial directory matching. We suspected that asking respondents to provide a telephone number had a negative impact on screener response rates, so we eliminated this question following wave 4, and telephone number was obtained solely through directory matching for waves 5 and 6. The survey datasets identify whether or not a telephone number could be identified through directory matching, but do not identify sample units that provided a telephone number in the screener survey. Since directory matching was the sole criteria used to identify telephone numbers for waves 5 and 6, we examined mail survey data for these waves to compare fishing activity between households with and without a telephone number. The comparison indicates that respondents with a matched telephone number reported fewer shore fishing trips (4.8 trips per angler) than respondents without a matched telephone number (5.4 trips per angler). While this difference is not significant, it is in the right direction to contribute to the differences in reported shore fishing activity between mail and CATI treatments and suggests that CATI estimates may be biased due to coverage error. We also note that differences in private boat fishing between respondents with

[17] Proxy reporting is permitted in the ABS CATI sample if the proxy respondent resides at the same addresses as the intended respondent, and he or she also is included in the topical ABS sample.

and without telephones are smaller; 4.1 trips per angler for respondents with a telephone and 4.3 trips per angler for respondents without a matched telephone number.

We cannot identify a single source of survey error that accounts for differences between CATI and mail estimates. Rather, we suggest that a combination of errors, including both measurement error and coverage error, are likely to contribute to the differences. We further suggest that estimates of shore fishing activity are more susceptible to error than estimates of private boat fishing due to differences in the nature of these activities. Specifically, private boat fishing requires greater investment in both time and money than shore fishing and is likely more memorable. Additional study is needed to more fully assess these hypotheses.

4.5. Coverage of State License Databases

An objective of this study was to assess the adequacy of state license databases for sampling recreational anglers. Previous studies (Brick et al. 2012b) suggested that a significant portion of recreational saltwater fishing trips are taken by unlicensed anglers. In the current study, we compared total effort estimates derived from the license samples to estimates derived from the ABS samples, which include all anglers, regardless of whether or not they had a fishing license (Table 10). These comparisons affirm the results reported by Brick et al. (2012) that total effort estimates derived from ABS samples are consistently larger than license estimates. Overall, ABS estimates were nearly twice as large as license estimates for shore fishing and 1.75 times larger for private boat fishing; differences between ABS and license estimates were larger for shore fishing than private boat fishing in each state. Differences between ABS and license estimates were largest in GA, where ABS estimates were nearly 5 times larger than license estimates for both private boat and shore fishing, and smallest in NC, where ABS estimates were approximately 1.5 times larger than license estimates for both types of fishing.

We considered different types of survey error to explain the differences between ABS and license estimates. An obvious source of bias in the license survey is coverage error resulting from anglers who fish without a license. Currently, all states included in this study require, with limited exceptions, a fishing license for anyone who participates in recreational saltwater fishing. Exemptions to licensing requirements include minors 16 years of age or younger, individuals who fish on for-hire vessels such as charter boats, individuals who fish from state-licensed fishing piers, and Florida residents age 65 or older (only applies to fishing activity within the state of Florida). Coverage error in the license survey could result from either exempted segments of the population as described above, or individuals who fail to comply with licensing requirements.

The survey instruments excluded charter boat fishing, and minors less than 18 were excluded from both sample frames, so exemptions for these categories of fishing are not likely to contribute to the observed differences between the two samples. The fact that differences between ABS and license estimates are larger for shore fishing than private boat fishing suggests that the license exemption for pier fishing is a source of coverage bias – there is no such exemption for any type of private boat fishing. However, we did not characterize different types of shore fishing activity, so we can't fully quantify the magnitude of bias resulting from this exemption. Finally, we explored the impact of the senior exemption in FL on license estimates by comparing the distribution of trips among age categories for the two samples. Based upon the license sample, approximately 2% of total fishing trips were taken by seniors older than 65. In contrast, seniors accounted for more than 23% of total fishing trips in estimates derived from the ABS sample. This suggests that the senior exemption in FL is a potentially large source of coverage bias in license estimates.

Effort estimates derived from license samples will also be biased if individuals fail to comply with licensing requirements. We attempted to characterize unlicensed anglers by comparing age and gender distributions across samples for those respondents who reported fishing during the reference waves. In each state, there were relatively more female anglers in the ABS samples than the license samples, suggesting that unlicensed anglers are disproportionately female. Comparisons of age distributions were inconsistent among states. In FL, the ABS sample includes a much higher percentage of seniors (65+) than the license sample – 24% vs. 2.7%. This suggests that senior anglers are more likely to fish without a license than younger anglers, which is consistent with the senior license exemption described above. This result contrasts with the characteristics of the samples in SC and GA, where younger anglers are more likely to fish without a license than seniors. Finally the age compositions of the two samples in NC are very similar; relative differences in age composition were less than 4% for all age classes. Coincidentally, ABS and license effort estimates are most similar in NC.

We also considered that differential nonresponse bias (avidity bias) may contribute to the observed differences in estimates between the license and ABS samples. Previous studies (Andrews et al. 2010; Brick et al. 2012b) demonstrated that households with licensed anglers are more likely to respond to a survey about fishing than households without licensed anglers and also more likely to report fishing during the reference period. Consequently, estimates of fishing effort will be biased if nonresponse weights are not adjusted to account for this differential nonresponse. We observed similar differences in response rates and topical survey eligibility between matched and unmatched households and adjusted nonresponse weights accordingly, presumably minimizing the effect of avidity bias on estimates generated from ABS samples. Consequently any residual avidity bias in the ABS sample would be limited to anglers in households that could not be matched to state license frames. It should be noted that estimates generated from license samples may also be impacted by avidity bias, although we suspect that the impacts are minimal, as described above. While we can't rule out nonresponse error as a potential contributor to differences between ABS and license estimates, it seems more likely that coverage error, resulting from license exemptions and illegal fishing activity, has a larger impact.

5. Discussion

Recreational saltwater fishing is a relatively rare occurrence among the general population, which presents challenges for collecting recreational fishing data in an efficient manner (Kalton and Anderson 1985; Lohr 2009). Results from this study continue to demonstrate that mail survey designs are feasible for collecting recreational fishing data, and that incorporating angler license databases into the sampling design provides a useful mechanism for increasing efficiency. The ABS sample frame provides nearly complete coverage of U.S. housing units, minimizing the potential for under-coverage error (Iannacchione 2011), and multi-frame designs can improve data collection efficiency, particularly when sampling rare populations (Lohr 2009). Final response rates for the mail survey treatment were higher than the CATI treatment and eclipsed CATI response rates after about three weeks of data collection, demonstrating that a mail survey design can match the current CHTS estimation schedule. Furthermore, the lack of differences between preliminary and final mail survey estimates provides assurance that preliminary point estimates, derived from partial survey data, will be similar to final estimates, produced after data collection has been completed.

While these benefits support further exploration of dual-frame, mail survey designs as a potential replacement for traditional RDD approaches, several challenges persist. For example, matching errors, resulting in misclassification of sampling domains, continue to be a challenge and are likely to result in biased estimates. More complete and accurate sample frames will minimize matching errors and decrease the risk of bias resulting from under-coverage and nonresponse. In addition, the present study achieved total response rates, considering both phases of ABS sampling, of 17.3%, which are only modestly higher than CHTS response rates. Including a cash incentive in topical survey mailings would likely have resulted in significantly higher response rates – previous MRIP pilot studies achieved total response rates of more than 30%, and a cash incentive increased screener response rates in the present study by 10-15%. However, the response rate is also an artifact of the two-phase ABS design, where total response rates are the product of screener and topical survey response rates. Brick et al. (2012b) proposed an alternative design that maintains the efficiency and coverage of the dual-frame, two-phase design but addresses concerns about matching errors and poor response rates. In the proposed design, address samples are matched to angler license databases by address and telephone number. Augmenting sample in this manner screens the ABS sample prior to data collection, effectively stratifying the sample into households with and without licensed anglers (Lohr 2009). This provides an opportunity to optimize sampling, making the data collection more efficient (Kalton and Anderson 1986). For example, addresses that match to license databases can be sampled at a higher rate than unmatched households, maximizing the collection of fishing information while maintaining the coverage of the ABS frame. Because the license information is only used to stratify the ABS sample, matching errors will only impact the efficiency of data collection; matching errors will not result in biased estimates. Brick et al. (2012b) also suggest collecting data from the ABS sample in a single phase, which would likely achieve considerably higher response rates than the two-phase approach. We would expect response rates similar to those obtained in the first phase ABS sample, all of which were over 40% when a $1.00 cash incentive was included in the survey mailing.

Finally, no comparison between data collection modes would be complete without a discussion about survey costs. The costs incurred for the present study do not provide an accurate representation of data collection costs for an ongoing survey due to the experimental nature of the project. Furthermore, we did not differentiate operational costs between the two survey modes. However, Link et al. (2007) reported that the operational costs of a telephone survey were 12% higher than the costs of a mail survey in an experiment comparing address-based sampling and random-digit-dialing. Similarly, deLeeuw (2008) suggests that mail surveys require fewer personnel than telephone surveys, which should translate into lower costs. We would expect the cost of a mail survey to be equal to or slightly less than the cost of a comparable telephone survey.

Specific conclusions and recommendations include the following:

1. Mail surveys are a feasible alternative to telephone surveys for collecting recreational fishing effort data. Mail surveys result in higher response rates than telephone surveys, and preliminary mail survey estimates can be generated in a timeframe consistent with the current CHTS estimation schedule.
2. Incorporating angler license databases into data collection designs increases the efficiency of recreational fishing surveys. While sample frames derived from license databases may be incomplete due to unlicensed fishing activity, samples of licensed individuals and households with licensed anglers are much more likely to report fishing than general population samples. Supplementing household samples with information from license databases should increase the efficiency of data collection while maintaining coverage of the entire population.
3. Frame matching errors are a recurring problem and potential source of bias in dual-frame sampling designs. Frame standards, which were implemented during the study, will help minimize matching errors.
4. Further study is needed to better understand the impact of data collection mode on survey measures. However, we hypothesize that differences between CATI and mail estimates are the result of recall error and coverage error, and that telephone samples are more susceptible to biases resulting from these errors. These hypotheses were not tested in the present study.
5. In the South Atlantic region, it is currently not feasible to sample exclusively from state license databases. In the present study, total effort estimates derived from license samples were considerably lower than ABS estimates. We attribute these differences to coverage error resulting from license exemptions and unlicensed fishing activity.
6. Cash incentives provide a substantial boost in response rates for mail surveys and should be considered in any mail survey design.

7. The single-phase ABS design with screening prior to data collection proposed by Brick et al. (2012) should be tested as an alternative to dual-frame designs. Such a design is not susceptible to bias resulting from matching error and is likely to result in higher response rates than the two-phase design.

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Tables and Figures

Table 4. Overall fishing prevalence - percent of respondents that reported fishing during the reference wave. For ABS samples, fishing prevalence represents both phases of sampling and is the product of screener eligibility and topical survey prevalence.

	ABS Sample		License
	Unmatched	Matched	Sample
Overall	6.6	21.9	37.2
State			
North Carolina	3.3	20.7	27.5
South Carolina	4.8	10.9	18.4
Georgia	2.2	6.3	11.2
Florida	10.5	29.0	50.4
Wave			
Wave 1	6.3	13.1	28.7
Wave 2	7.3	20.4	36.4
Wave 3	7.6	30.8	39.0
Wave 4	6.0	23.6	47.9
Wave 5	6.3	21.4	39.8
Wave 6	5.7	23.7	36.7

Table 5. Estimated and actual number of addresses with licensed anglers for each state and wave. Estimates are significantly different from actual counts in every comparison except wave 2 in North Carolina at the α=0.05 level.

	ABS Sample Estimate (000's)	License Frame (000's)	Ratio of ABS to License	95% CI of Lower Limit	95% CI of Upper Limit
North Carolina					
Wave 2	423	406	1.04	0.96	1.13
Wave 3	401	363	1.10	1.01	1.20
Wave 4	251	185	1.36	1.21	1.51
Wave 5	386	328	1.18	1.08	1.28
Wave 6	260	218	1.19	1.07	1.31
South Carolina					
Wave 2	227	264	0.86	0.79	0.93
Wave 3	231	273	0.85	0.78	0.91
Wave 4	228	170	1.34	1.24	1.45
Wave 5	195	153	1.27	1.16	1.38
Wave 6	198	153	1.29	1.18	1.41
Georgia					
Wave 2	167	131	1.28	1.12	1.45
Wave 3	123	103	1.20	1.02	1.37
Wave 4	226	99	2.29	2.04	2.54
Wave 5	227	111	2.05	1.83	2.28
Wave 6	250	126	1.99	1.78	2.19
Florida					
Wave 2	190	831	0.23	0.19	0.27
Wave 3	742	640	1.16	1.06	1.26
Wave 4	848	571	1.49	1.36	1.61
Wave 5	981	621	1.58	1.46	1.70
Wave 6	1,130	715	1.58	1.47	1.69

Table 6. Mixed mode survey response rates by domain[18].

	ABS Sample			License
	1st Phase	2nd Phase	Overall	Sample
Overall	37.3	46.4	17.3	33.1
State				
North Carolina	36.9	49.1	18.1	34.5
South Carolina	36.2	47.2	17.1	36.0
Georgia	40.0	42.8	17.1	32.6
Florida	39.4	46.3	18.2	31.7
Wave[19]				
Wave 1	27.7	55	15.2	32.9
Wave 2	32.1	45.1	14.5	32.8
Wave 3	31.6	48.8	15.4	33.0
Wave 4	43.2	46.7	20.2	33.2
Wave 5	45.1	39.9	18.0	33.2
Wave 6	44.1	42.7	18.8	33.4
License Match				
Match	43.0	50.7	21.8	NA
No Match	36.8	45.5	16.7	NA

[18] The matched domain includes addresses that could be matched to angler license databases. Addresses in the unmatched domain could not be matched to a record in a license database.
[19] A $1.00 cash incentive was included in the initial 1st phase ABS mailing beginning in wave 4 and continuing through wave 6.

Figure 1. ABS Screener response rates (AAPOR RR3) by survey wave.

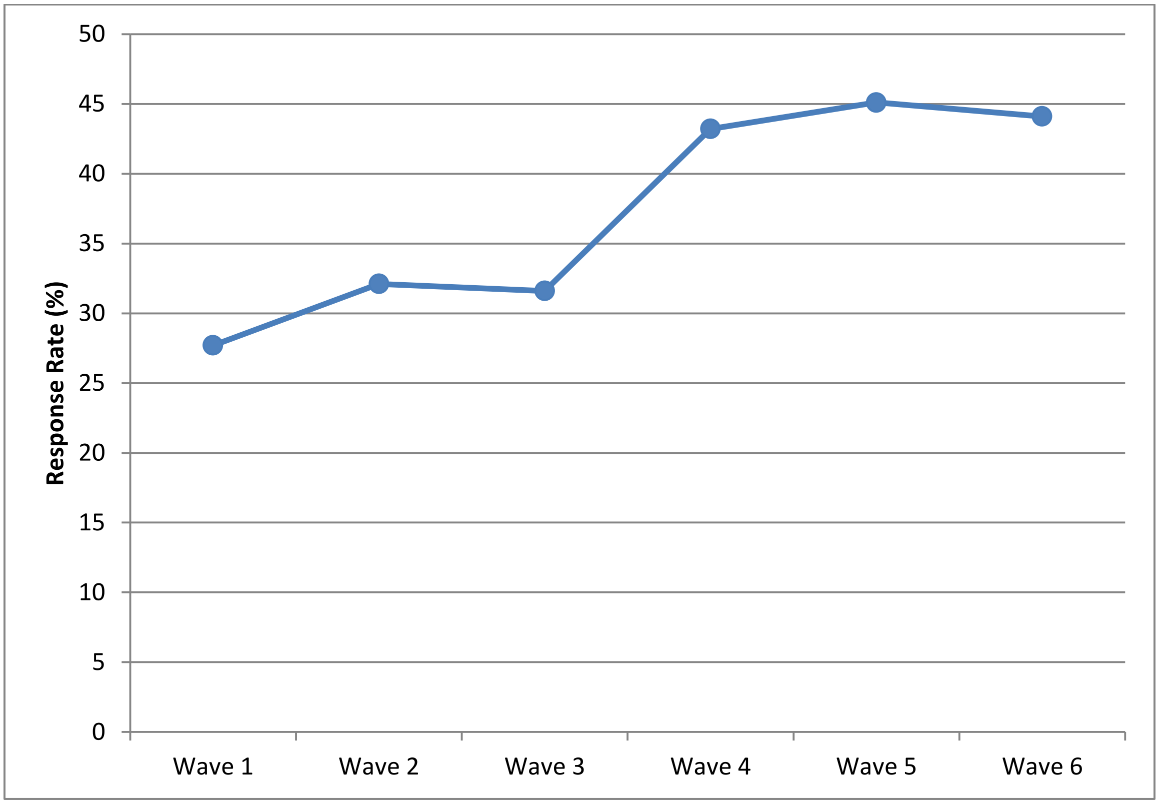

Figure 2. ABS screener survey – cumulative distribution of completed surveys over time.

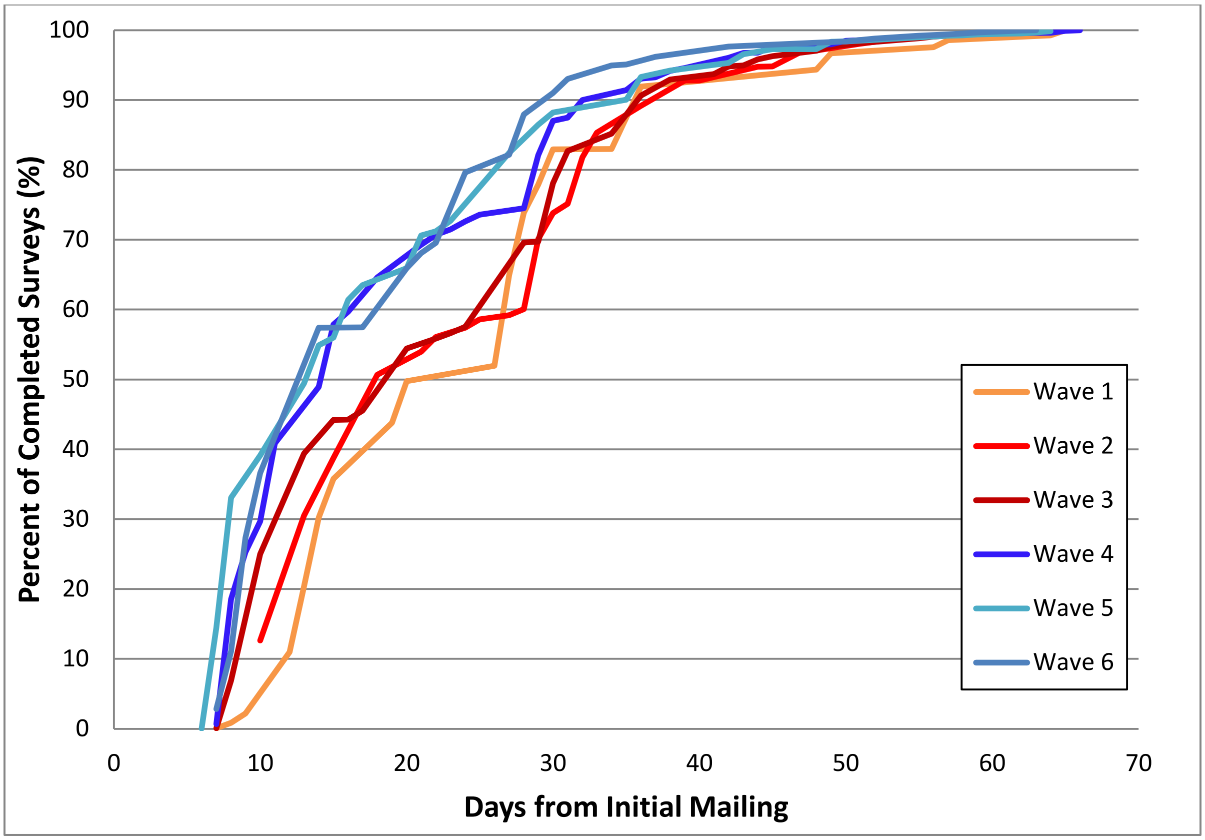

Table 7. Preliminary[20] and final overall response rates by data collection mode for the ABS and license sample data collections. Response rates for the ABS sample reflect response rates for the two phases of sampling.

	CATI %	Mail %	Total %
ABS Sample			
Preliminary	14.3	13.1	13.6
Final (all sample)	14.4	19.2	17.3
Final (with phone match)	13.4	22.0	16.9
License Sample			
Preliminary	27.4	22.4	24.8
Final	27.4	38.1	33.1

[20] Preliminary response rates reflect complete CATI data and mail surveys that were returned within three weeks of the initial survey mailing date.

Figure 3. Cumulative mail survey response rates for the second-phase ABS (blue) and license (red) samples over all states and waves. Dashed lines represent the final CATI response rates for the respective sample frames.

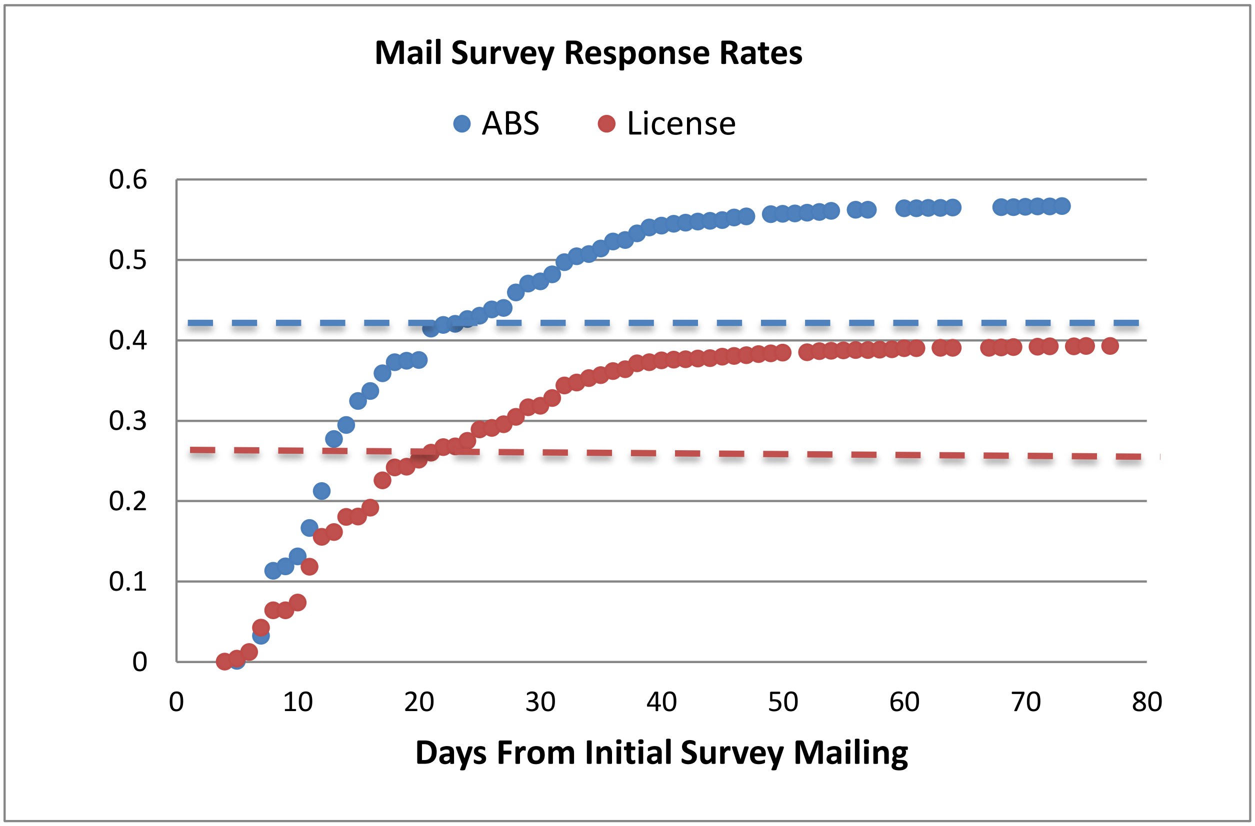

Table 8. Preliminary[21] and final estimates of fishing prevalence and mean private boat and shore trips per angler for anglers who reported fishing in the mode during the reference wave for the ABS, mail sample.

	North Carolina		South Carolina		Georgia		Florida		Overall	
	Prelim n=631	Final n=934	Prelim n=649	Final n=955	Prelim n=357	Final n=533	Prelim n=849	Final n=1,214	Prelim n=2,486	Final n=3,636
Fishing rate	17.2	18.2	24.5	12.8	12.9	11.7	31.9	33.6	27.0	27.6
Mean boat trips	3.7	4.1	3.1	3.9	3.5	4.1	4.7	4.7	4.4	4.5
Mean shore trips	5.1	4.9	4.5	5.5	3.4	3.7	6.6	6.8	6.0	6.2

[21] Preliminary and final estimates were derived from mail surveys that were returned within 3 weeks and 12 weeks of the initial survey mailing, respectively.

Table 9. Estimates of fishing activity by state and data collection mode for the final second-phase ABS and license samples. Mean shore and boat trips are for individuals who reported fishing in the mode during the wave. Significance: * indicates a significant difference (*p*<0.05) between CATI and mail estimates

	North Carolina		South Carolina		Georgia		Florida		Overall	
	CATI	Mail	CATI	Mail	CATI	Mail	CATI	Mail	CATI	Mail
	n=596	n=934	n=538	n=955	n=346	n=533	n=661	n=1214	n=2141	n=3636
ABS Sample										
Prevlance	21.9	18.4	19.4	23.9	13.4	11.7	37.0	33.4	29.0	26.7
Mean boat trips	4.2	4.1	3.9	3.7	3.0	4.1	4.7	4.7	4.5	4.5
Mean shore trips	3.9	4.8	3.9	5.0	3.2	3.6	5.1	6.1	4.6	5.6*

	North Carolina		South Carolina		Georgia		Florida		Overall	
	CATI	Mail	CATI	Mail	CATI	Mail	CATI	Mail	CATI	Mail
	n=887	n=1330	n=906	n=1433	n=886	n=1213	n=797	n=1234	n=3476	n=5210
License Sample										
Prevalence	26.0	29.0	14.0	22.4*	10.4	12.1	45.2	55.1*	33.5	40.9*
Mean boat trips	4.4	4.3	4.9	4.6	3.8	4.5	4.4	4.6	4.4	4.6
Mean shore trips	5.3	5.1	4.6	4.8	4.1	4.6	6.2	5.3	5.9	5.2

Table 10. Estimates of total fishing effort, in thousands of angler trips, for shore and private boat fishing. ABS estimates represent total fishing effort, including fishing by both licensed and unlicensed anglers. License estimates represent fishing activity by licensed anglers only. Percent coverage is the ratio of license estimates to ABS estimates.

	Private Boat			Shore Fishing		
	ABS	License	ABS:License	ABS	License	ABS:License
North Carolina	2,409	1,596	1.51	3,689	2,420	1.52
South Carolina	1,405	855	1.64	1,909	781	2.45
Georgia	1,166	240	4.85	1,223	251	4.88
Florida	19,139	8,487	2.26	20,127	8,209	2.45
Overall	24,119	13,836	1.74	26,948	13,811	1.95